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Leroy Stevens Oral History

Leroy Stevens

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Oral History Collection

Interviewer's Comments

Narrator's Name Dr. Leroy Stevens

Interviewer's observations about the interview setting, physical description of the narrator, comments on narrator's veracity and accuracy, and candid assessment of the historical value of the memoir.

NOTE: Use parentheses () to enclose any words, phrases or sentences that should be regarded as confidential.

Dr. Stevens seemed remarkably nervous throughout our taping, commenting several times that he felt he had nothing to say. In fact, he did, but only in a corroborating way: his tape confirms much said by Russell, Snell and other old-timers. Interestingly, Stevens represents an age group, or "generation" with few members left at Jax: the immediate post-World War II group hired by C.C. Little just as federal funding began to be a feature of American science. Stevens makes reference to five or six scientists hired when he was, who are no longer at Jax.

In terms of anecdote, or vignette, this tape is thin. Since Stevens was full of such stories after I had packed up the recorder and was on my way out, I imagine fear of the microphone paralyzed his memory. Like everyone on the staff, Stevens was aware of the differences in style and personality between C.C. Little and Earl Green. As do many of the scientists I interviewed, Stevens tends to his science, holed up in his little corner of the Lab, and willingly forgets the outer world.

A picture of the intra-Lab social scene is provided here, in passing, in Steven's reference to the "jack-ass" club.

I have no reason to question Stevens' veracity or reliability, but since this tape says so little and nothing not stated elsewhere and in more detail, its veracity is not an issue.

14 June 1986
Date

Susan Mehrtens
Interviewer's name

Oral History Collection

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Place Bar Harbor

Date 11 June 1986

Leroy C. Stearns
Narrator

Susan McIntire
for the Laboratory

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Stevens

Interviewer's Notes and Word List

Rochester
Curt Stern
Don Charles
C.C. Little
Bill Murray
Tibby Russell
Jean
Buffalo
Michigan
Maine
Prexy
Meredith Runner
Dr. Little
Boston Brahmin
Earl Green
Rich Prehn
Hoxie
Ham Station
Jack King
Carl Cohen
Bill Mellington
Marion ("Teak") Drasher
Morris Smithberg
Andy Kandutsch
Doug Coleman
Chai
Hans Holtfreter
George Snell
Katrina Hummel
Elizabeth Fekete
Nobel
Don Bailey
Seldon Bernstein
Pete Hoppe
Wes Whitten
Charity (Waymouth)
Barbara Sanford
Francis Schmitt
MIT
Henry Winn
Massachusetts General Hospital
Harvard

Terms

paramecia
teratoma
testicular teratoma
strain-129
parthenogenetically
micromanipulator

The Jackson Laboratory
INTERVIEW DATA SHEET

This section is to be completed by the Interviewer.

Narrator Dr. Leroy Stevens Address The Jackson Lab Bar Harbor Me Phone 207 288 3371
 Birthdate _____ Birthplace _____ Interviewer Dr. Susan Phelan Phone 207 244 7353
 Date(s) & Place(s) of Interview(s) 11 June 1986 Bar Harbor
 Collateral Material Yes _____ No X Terms unrestricted

 Complete each of these sections as the tape is processed in each step.

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					Begun			
					Text finished			
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Oral History Collection

Collateral Materials Report

Narrator's Name Stevens

Collateral materials, whether originals or copies, enhance the value of an oral history memoir. Ask the narrator if you may borrow or keep such things as personal photographs, newspaper clippings, pages from a diary, and other mementos. Borrowed materials can be photographed or duplicated and then returned.

List and describe all acquisitions below. A typical description might be "Copy of letter from Governor Henry Horner to James L. Singleton, February 29, 1937." Provide as much identifying information for each photograph as possible. Each photograph should be labeled on its back as well as listed below.

1. None

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This is the tape of an oral history interview of Dr. Leroy Stevens, given as part of the Jackson Laboratory Oral History Project, sponsored by the Acadia Institute. This interview was held on June 11, 1986 in Dr. Stevens' office at the Jackson Laboratory, in Bar Harbor, Maine. The interviewer was Dr. Susan E. Mehrtens.

SM: I will begin by asking you when you first heard about the Jax and how?

LS: I was a graduate student at the University of Rochester and the emphasis at that university at that time was genetics because one of the world's foremost geneticists, Curt Stern, was chairman of the department when I was there

SM: It is always that way.

LS: There were several geneticists in the department of biology at the University of Rochester and a couple of them had visited to work at the Jackson.

Lab. And they spoke very glowingly of the place.

SM: When would this be now, the late '30's?

LS: No, this would be not too long after the war. About '47, '46 or '47. Somewhere around there. Now, as I said, some of the people there had spent time up here. That is when I first heard of the Lab. Oh, also I first heard of it at the

time of the fire. I recall very clearly seeing it on a newsreel, pictures of the Lab burning. That was quite a memorable event. I also heard of it because of a very important discovery that was made here at the Lab. And that is the inheritance of the mammary tumor virus. It was the first case in mammals where non-chromosomal inheritance was discovered. It had been known in paramecium but it was a very important discovery and one which received a lot of attention. And that was a result of a paper that appeared in Science. The authors of the paper were "the staff of the Jackson Laboratory." The whole Lab was much smaller then, of course. All of the staff members were involved in this discovery of mammary tumor virus which eventually led to the theory of the causation between virus and cancer in mammals.

SM: Now, how was it, you were then a graduate student, how was it that you wound up working here?

LS: Well, my degree was in experimental embryology using amphibians. Jobs were very hard to get then, really hard. Nothing was available and the chairman of our department, who at that time was Don Charles, had worked at the Lab and his friends were some of the staff members. He went to a meeting of the Genetics Society of America and there he met C. C. Little, Dr. Bill Murray and Tibby Russell. And he asked if there were any openings at the Lab and they said there was. And so, Tibby Russell and Bill Murray

came to the University of Rochester to interview me. And as a result of that, I had a job offer.

SM: But you had not really seen the Lab yourself?

LS: I had seen it, yes. My wife and I spent a vacation up here one time and we drove by the Lab and Jean said, "Wouldn't it be nice if you could get a job here?" And I said it would be great. But I never dreamed I would wind up here. I came here as an assistant to Dr. C. C. Little and I took over some projects that he had going.

SM: Now when you came here, you had vacationed here and you thought it would be a nice place to work.

LS: Yeah.

SM: But did you ever really think, you and your wife, of what it meant to work in a rural place? Did you ever feel like this was the middle of nowhere and-

LS: Not at all, as a matter of fact, I was born and brought up in suburbia and I always felt that I was being cheated. You know when I was a very young kid. I wanted very much to live in an area where there were lakes and hills and forests and things like that especially the sea. So we certainly thought we would like living here. Jean always lived in fear that I would become a professor at the University of Buffalo or something. I wouldn't like that either. My wife has relatives here, so she had been up here.

SM: 'Cause it is a theme that runs through many of the interviews

I have done, that people come here and then discover they can't- it's too quiet, you know, there is nothing to do, so there-

LS: People either like it very much or they hate it.

Fortunately for us we like it here very much.

SM: So you took over some of C. C. Little's projects. What developed then, I mean these things just sort of evolve, I guess.

LS: Well, C. C. Little was a very informal person. He was called Prexy, because he was President of the Universities of Michigan and Maine. First Maine then Michigan. And I just happened to discover what has kept me busy for the past 35 years. The project I was working with for Prexy was involved with a study of mutation rate which involved looking at very large numbers of mice looking for differences from normal. And to try to find out if they were inherited or not. I found a mouse with a tumor--it is called a teratoma which interested me because it was the first one that anyone had ever seen-- this testicular teratoma. And this tumor is very much like an early embryo. The tumor is composed of many different kinds of cells and tissues, unlike other kinds of tumors that are usually composed of one or two kinds of cells. Teratomas are composed of muscle, nerve, brain, fat, glands, all kinds of things, and at that time there had never been an experimental animal to work with to investigate that tumor

and- so anyway I found this one tumor that was quite interesting, the first one anyone had ever seen in a mouse. And after autopsying about five hundred more mice, I found another one. And another, and it soon became apparent that about 1% of this strain-129 had spontaneous testicular teratomas. And so I was very anxious to work with this tumor because I am an embryologist and these tumors resemble normal mice except the tissues are all mixed up. So at a Christmas party I asked Prexy if he would approve of my applying for a grant to the American Cancer Society. And he was very agreeable to this because he thought that these tumors were pretty interesting too. This happened shortly after I arrived here that I found teratomas. Maybe it was six months after I had got here. And I have been busy with these tumors ever since. Probably will be for a while.

SM: So, it struck me that the Lab was quite good about people sort of bumping into things and then having the freedom to pursue them.

LS: That was a really wonderful thing as far as I was concerned. I felt perfectly free to do anything I wanted. And I didn't have to account to anybody. I very slowly got this thing off the ground. I mean, what do you do when you find something as rare as that. How can you take advantage of it and so on. I did a lot of genetic manipulation trying to increase incidence of teratomas and now

LS

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we can experimentally produce them 100%.

SM: Wow.

LS: But it was a long time getting going. So that is how I happened to stay here. I originally had planned to teach, get more research experience here, but after finding these tumors, I was fascinated by them.

SM: Do you ever regret the fact that you don't teach or do you find enough in the summer program or-

LS: Not at all. There is plenty during the summer program. We have a course in mammalian genetics and I lecture at that every year. We have students in the labs and you get invited to give seminars at other places and I don't regret not having taught. I did enjoy it while I was in graduate school.

SM: Do you ever find yourself lonely, in the sense that you don't have other embryologists to talk to?

LS: Not really. I had other embryologists to talk to, for example, when I first came here there was a guy by the name of Meredith Runner. He occupied this lab by the way. He was the first person to occupy it. He was an embryologist and it was great talking with him. There are other people who are interested in development and morphology and of course, genetics. And so I have always felt that I had plenty of people to talk with. My work has always been quite simple, that is, almost anybody could understand it. Furthermore, a fellow graduate student, Maurice Smithberg, came here at about the same time I did. We had both been students of Holtfreter at Rochester.

SM: Well, I am not sure that is so. What have been some of the rewards of working at the Lab? You have mentioned one, the fact that you have such freedom.

LS: Yes, that was great. I have had a great deal of satisfaction from my work and I ...figured out that about every five years we make a breakthrough of some kind which sustains you for the next five. And this periodic jolt or shot in the arm is great, exhilarating. And it is a lot of fun to, after you find something like that to, iron it out. I don't know what has happened to my voice.

SM: Are there frustrations that you meet here too, the opposite side of the coin?

LS: Well, when you are doing something like I have done, that is entering a whole new area, many times, what do I do next, you know. And I have done everything I can think of and most of them don't work, which is frustrating. However, there are enough that do work to keep going.

SM: How did you think, in terms of your own science and what you do, how do you think the Lab has changed over time, say with the new Directors coming and going, the Lab getting bigger?

LS: It has become much more formal. It used to be, very much a family affair. There were fewer staff members and Dr. Little was not a very well organized man. And, oh for example, they could find no proof of what my rank was.

LS

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SM: Did you really have ranks though? Did the early lab even have ranks?

LS: Well, yes, but at first no. I think everybody was a research associate. Yeah, I remember when I was promoted after about three years, I was promoted from laboratory fellow to research associate and Dr. Little came in and told me about it. And I asked him what that meant. And he said, it means that we would like to have you stay here for as long as you want which was a fantastic feeling which the younger staff members don't have now. It is too bad in a way because it really gave you a good feeling that you could do whatever you want and you wanted to accomplish something, of course. But now, I think the younger staff members not hired permanently for quite a long time. And they are very selective about who is given permanent staff position.

SM: Well, I think times, economically speaking in the grant world and all-

LS: Yes.

SM: are much tighter.

LS: It is much more competitive now than it used to be. I think a lot of it used to be that Prexy's personal association with people in the granting agencies had a lot to do with it. He used to travel a lot and he was in close touch with people in the American Cancer Society and also people at NIH. And I think that his personality helped a lot to get me supported.

I never could get it now in this competitive day. If I had proposed what I proposed then, now, it would never have been...I am sure.

SM: Interesting. So initially he basically got the money to support you as opposed to your having to get your own grant.

LS: Right. There was both, of course. But he had many influential friends. His personality was a tremendous help to researchers at the Lab.

SM: Well, people describe him as almost charismatic.

LS: He was, ya. He was a really great character. I mean you could tell that he came from the best of the Boston families. He was Brahmin. But on the other hand he was a very, very warm person, very easy to get to know, very emotional. I have seen him almost break up many times. I really have. He was a really emotional person. I can remember feeling perhaps that that was one of the functions of the Director to create a little discord among the staff so they would have some feeling of unity. But, the next Director, Earl Green, was completely different. Earl was extremely well organized. It was just what we needed. I think we had the type of Director we needed ever since I have been here. It was best for that time. Originally, Prexy was the personality and he established the Lab as a scientific institution. Then Earl Green came with his very excellent sense of organization.

LS

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SM: And helped stabilize the Lab.

LS: Yes.

SM: An institution can't go indefinitely in a sort of helter-skelter fashion.

LS: No.

SM: Now, do you think, also, in the sense of timeliness that Rich Prehn was appropriate for his time?

LS: Ya, I think Rich was very good.

SM: My sense is now after doing seventeen interviews, that a lot of people were beginning to feel too organized at the end of Dr. Green's tenure.

LS: People did feel that way.

SM: And that-

LS: Shall I see if we can go in the lab. It's a little-

SM: No, this is fine. This is real fine, I think. And I think that from what I have heard Dr. Prehn was consciously chosen to have a different style than Earl Green.

LS: Uhuh. He was looser. He was looser than Green.

SM: But in all this time, your science wasn't particularly affected by the changing of Directors.

LS: Not at all, fortunately, I think. Now for example, I have had uninterrupted grant support ever since I have been here. And this is great. To spend your energy doing science instead of of writing applications and worrying about getting support. And I think that must be a very uncomfortable feeling. I have

LS

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never had to have that. I mean, I have always had to reapply for my grant, but I got five years at a time, so I didn't have to worry more than once every five years.

SM: Now when you apply for grants, does the Lab give you support in the sense of writing and secretarial support and-

LS: Oh, yes. You write your own, of course, but the secretarial support is excellent.

SM: Are there any particular memorable events you have of these years, funny stories, or mishaps, or-

LS: There are many memorable things, of course. This is what I mean when I said I am not very good at this.

SM: Do you remember, Tibby Russell spoke about this, there was an appeal made to the Jackson Lab apparently, at some point, probably from some group from Washington to investigate the claims made by Hoxie.

LS: Oh yeah-

SM: And they did this. He had some-

LS: Kind of a box that he put-

SM: That's right, that's right, and claimed there were these energies that were creating cancer and people had to be protected from these energies and so apparently there were all kinds of studies with the mice to see and - then at the end of it all they had a party. Do you remember this?

LS: I remember that.

LS

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SM: Now, was Tibby the mouse.

LS: Yes, she was the mouse. Charity showed me the picture that was taken at the party. Bill Murray headed up the experiment and we had a very funny party afterwards in which we were having a mock trial. It was fun.

SM: And you were in on that.

LS: Ya.

SM: A lot of people remember that.

LS: Ya, It was fun.

SM: And then they talk about having Christmas parties.

LS: Oh, ya.

SM: Where children would come and they would see Santa Claus.

LS: Right, they still do.

SM: Oh, really.

LS: Oh ya.

SM: How wonderful.

LS: They always had Christmas parties. Also, we had informal parties, maybe once a month or something like that where everybody in the Lab would come and there would probably be about thirty people or so at that party.

SM: And everybody knew everybody.

LS: Everybody knew everybody, unlike now. I don't even know some of the staff members, I am ashamed to say.

SM: Well, it is understandable if there are hundreds of people.

LS

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LS: Yeah, there are now about five hundred people here.

SM: Incredible. But they do speak definitely of that Hoxie party.

LS: That's funny.

SM: And they speak of the Christmas parties. Many of the scientists recall the days when they had a big debate about using radioisotopes.

LS: Oh, God, yes.

SM: And how dangerous this was going to be.

LS: Right.

SM: And they exiled people who were the early pioneers -

LS: Now it turns out we are sitting on a bed of granite which is quite radioactive.

SM Yeah, that's true. But were you one of the ones that- how did you feel about bringing the isotopes into the Lab?

LS: I had no interest in it. I didn't know how I would be able to use them. So, I thought it was a good idea because how can scientists here compete with other scientists if they don't have the wherewithall to do it? I thought it was an excellent idea. But I never used it...

SM: The question I have gotten on some things that have come up fairly consistently in these interviews have been the Lab's concern for the mouse colony, making it very careful about what is brought in here.

LS: Oh ya.

LS

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SM: Have you ever had a problem with your own research because of that?

LS: No. Once I wanted to work with rats and that was a drag. We had to send a taxi out to the airport to get the rats and we had to keep them at Ham Station about seven miles away. We had to go out there, my assistant and I, we would have to take a shower before we went in and a shower when we came out. It was a drag, very inconvenient. But it was possible.

SM: Now, did you have to do this indefinitely or was there a certain...

LS: Ya, we had to do it every week. We had to go out there every week until we finished the project. But I think it is worth it, the inconvenience was worth it and it does put some people at a great disadvantage. As I said, you don't have to do this other places and so we are disadvantaged by having this arrangement. But that will be overcome.

SM: Why?

LS: They plan to have new importation facilities. Also they had talked about having buildings, other buildings which we could import mice into ... Mice and biological materials. But it has never bothered me that much.

SM: I think some people it would bother - or affect their research more than others.

LS: Ya.

LS

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SM: You talk about the importations facility building, how large would you like to see the Lab become?

LS: I have always liked it the way it was.

SM: Yes, everybody does.

LS: Do they?

SM: Yes, yup.

LS: I think it is fine now. I mean you are always associated with a comparatively small group of people even in a big place. You select your own associates. People you want to interact with. That doesn't really matter how big it is. But there was a very close feeling, I think, between the staff members and undoubtedly there are now. There were several people of my own age, who came about the same time I did.

The situation is similar to what it is now with a lot of young scientists here at the Lab and I think that is real good because you form very close friendships.

SM: So, who came with you, who would be your contemporaries?

LS: Jack King, who was an ecologist. He came shortly before I did. Carl Cohen came shortly before I did. Bill Millington came shortly before I did and, as I have mentioned, Maurice Smithberg, Teak Drasher, Marion Drasher, she was one of the first molecular biologists here. She was actually into DNA as it was just becoming fashionable.

LS

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SM: Now of all these people, not one of them is here still.

LS: Right, right.

SM: That's interesting. Because there's a variety about well, maybe ten years later, like Andy Kandutsch and-

LS: Andy came about the same time as I did. And Doug Coleman came not too long after I came. And Chai. Chai has retired now but Doug and Andy are still here.

SM: Right.

LS: They are younger than I am but I was a little late getting out of graduate school because I was involved in the war. That was three or four years out of my career.

SM: Right. So there really haven't been other places that you worked except as a graduate student.

LS: That's right, that's right.

SM: This is very true. I am finding it very hard, I will have some younger people who have worked elsewhere, but it is not been the norm to find people who have worked around and then come to Jax. They seem to come and love it so much that you stay forever. It is interesting. It is hard to get comparisons.

LS: Right. As I said, the biology department at the University of Rochester was an excellent department because it had Curt Stern and also my boss, Hans Holtfreter, who was one of the most experimental embryologists at that time. So I came from a very strong place to a place I felt was very good.

LS

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SM: Well, it had potential. It was strong by that time.

LS: Ya, ya.

SM: It had a full decade to establish itself.

LS: Oh, ya, it was strong. Tibby Russell, for example, was one of them and of course, George Snell and -

SM: Did you ever work tangentially with any of these people?

LS: When I first came here, I worked with--in the laboratory of Katrina Hummel and have you interviewed her?

SM: No.

LS: You will find that a challenge. I worked in the laboratory of Katrina Hummel and Elizabeth Fekete who was a morphologist. They were both very supportive of my work and Tibby Russell and Meredith Runner was also very supportive. And other people.

SM: I know, why I ask about George is that I read his papers when he won the Nobel Prize he gave an address, and I read his address, and he cited several of your papers.

LS: George Snell?

SM: Yes.

LS: I once did an experiment with George.

SM: But you were pretty much on your own?

LS: Ya.

SM: That's wonderful. Can you assess the Jax in terms of its strengths and weaknesses? It certainly is a strength in terms of it giving you the freedom to pursue what you wish to pursue.

LS: Right. That is a tremendous strength. To be able to do whatever you feel like doing is really good and I think I would never have accomplished what I did if I hadn't had that feeling that I could do whatever I wanted. Then if it worked, great, if it doesn't work, then it's too bad. But I think one of the weaknesses of working here is the fact that you have to get your own support. That is absolutely... everybody has to get their own support and sometimes that is very difficult. It hasn't been for me but it has been for other people and it is just because competition is so incredibly strong. Yeah, I think that would be very uncomfortable to have to wonder how long you are going to be able to keep your job. I have always been a lucky guy and I haven't had to worry about that.

SM: Now, do you think that is because you happened to luck into a topic that was highly fundable?

LS: Ya, I think so. I think as far as I am concerned there is an enormous amount of luck involved in research. Very, very much so. Even though when I first started this tumor was extremely--nobody had ever seen one before, and nobody has ever seen one since, except in my mice. And, you know you have no idea of what am I going to do next, how am I going to study the embryology of this tumor, find out where it comes from and so on. I just tried everything I could think of and every once in a while something would work and ... be able to carry on from there. And I have been working on it for

thirty-four years so I am due for some more luck now, I say.

SM: About time, huh.

LS: Ya. Well, you have to be alert to- if something happened, for example, a mutation occurs in your colony. It is luck, and I have had a couple of these happen which have changed the course of my work. I figured out a way to experimentally induce these teratomas, so I can do it like that any time I want. And that was luck. I mean, I tried a lot of different things that didn't work.

SM: People think here that the Jax scientists are divided into two groups. There are the mouse geneticists or the mammalian geneticists and then there is everybody else. Do you see a division like that?

LS: No I don't. As a matter fact I think that everybody here uses genetic techniques. I mean, I am an embryologist but I have used genetic techniques to find out what I have been able to about teratomas. But I don't consider myself a geneticist, but I use genetics all the time. It is really great to be in an environment like this where you have so many really good geneticists. They have developed techniques and methods that have been extremely useful to me. For example, George Snell developed congenic inbred strains and you don't have to be a geneticist to use this as a tool. And I used it as a tool to discover quite a bit about teratomas. Don Bailey invented

recombinant inbred strains. And here again, you don't have to be a geneticist to develop them, but I have found them extremely useful. So I don't consider myself a geneticist; however, I use genetics all the time. I am basically a developmental biologist or embryologist. And I am interested in early mouse embryology. And I have found that - I have been able to look at the earliest stages of teratomas and what happens is that the cells in the testes, a cell in the testes thinks it is a fertilized egg and it begins to develop. It develops quite normally for a while and then it becomes completely disorganized so you get a mish-mash of a great variety of disorganized tissues. But it originally starts out like a normal mouse embryo. And then another piece of luck I had, Seldon Bernstein came about the same time as I did too. Seldon one day found a mouse that had an ovarian teratoma and he knew me well enough to know that I would be interested. So he gave the mouse to me and I started a colony and I found that we had a very obscure inbred strain in which half the females would have ovarian teratomas by the time they were three months old, which was a great shot in the arm for me. Because I worked with testicular teratomas for a long time and I found that they came from germ cells, cells that would normally would develop into sperm. And when I started looking at the development of the ovarian teratomas, I found that they are derived from eggs in the ovary which begin to develop

parthenogenetically. And they developed beautifully for quite a while. They looked just like a normal embryo and - but then they would all get mixed up. They became disorganized and you had a tumor instead of a baby mouse. But I worked with ovarian tumors for a long time and we found some very interesting things. For example, in this strain of mouse that have a lot of ovarian teratomas, another interesting thing happens. It is very closely related. If you autopsy a large group of virgin females, about 10% will be pregnant. And what happens is that the eggs get ovulated, they get out the ovary, get in the oviduct and some of them think they are fertilized and begin to develop parthenogenetically.

SM: Wow.

LS: And they develop very well for quite a long time until they get into the uterus and they implant in the uterus as does a normal embryo and up to this stage it is just like a normal mouse embryo. But right after implantation, they die. And a very interesting thing is that somehow the uterus knows that there is something funny here. Even though they looked identically like normal embryos. We did an interesting experiment. If you take a, say four-cell mouse embryo, it is enclosed in a non-living membrane and you can dissolve that membrane off and have these four cells. The parthenogenetic strain it is a black strain, and there is a very neat trick that, if you take off this outer membrane, these

cells are very sticky. So this method was developed somewhere else but we used it. So what we did, was to take a parthenogenetic embryo, which are very easy to get with spontaneous parthenogenesis in this strain, and then take a four-cell albino embryo and, as I said these cells are very sticky, push them together they will stick together. And then they will round up as a single big embryo and then you can take it and transplant it to the uterus of a pseudopregnant female and you can get baby mice that are made partly of black - the parthenogenetic genotype and partly white. All of the cells and tissues are complete mixtures of two different genetic situations. And we were able to get white mice with black spots and the black spots can be only derived from parthenogenetic embryos which proves that cells of parthenogenetic origin can participate in normal development and also we did several biochemical analyses so we could identify the source of the cells in any tissue. And in all We had one mouse--it was white with black spots and she produced both black and white babies. So the eggs are derived some from the parthenogenetic embryo and some from the normal.

SM: That is amazing.

LS: This is the eye of one of these mice with a pink and black iris. This mouse has three parents, two mothers and one father. The black part is derived from the parthenogenetic embryo and the pink part

is derived from the normal, which proves that parthenogenetic cells can participate in normal development. So the question still is- how does the uterus know that they are funny?

SM: You haven't solved that?

LS: No, I have not solved that yet. Pete Hoppe is working some on this. He thinks possibly there is an immunological basis for it...

SM: Now, is Peter here at the Lab?

LS: Yes, Pete's here, ya. Pete's been here for about eighteen years, I think. And he is an embryologist and I interact a lot with Pete.

SM: Now, did you want him - how was he hired?

LS: Pete?

SM: Hired as an embryologist, were you really eager to have another embryologist?

LS: Sure, oh ya. He came to work with Wes Whitten who was also an embryologist. And I interacted a lot with Wes. Ya, that's one great thing about the Lab, there are many kinds of expertises available to you and you can interact if you want, if you don't, you don't have to. There is no restriction on whom you associate with.

SM: People say that is a very good thing about the Lab.

LS: It is a very good thing about the Lab.

SM: And they wouldn't want to see it rigidly departmentalized.

LS: Right, right. Ya, you can, when you meet people at parties most of the time you are talking about science. Very many collaborations get started that way, which is very good.

SM: Now, has the Lab supported you in the sense of large equipment that you share with other people?

LS: I have never needed that. I have always been able to buy equipment if I needed it. For example, a micromanipulator which costs about \$7000. And we are going to buy there is an apparatus now that can fuse cells electronically and we are going to buy one of those. And other people in the Lab will be able to use it.

SM: Now, this is all on your own budget.

LS: Yes, but if I did need any large equipment, expensive equipment, I would feel as if it would be available to me. But I have never needed it.

SM: You have never used things like the electron microscope.

LS: Just a bit, just a bit but -

SM: Some scientist say it is really only, say, in the last decade that the Lab has made a concerted effort to try to supply some of these enormously expensive things, that individual grants can't come up with. Can you think of some of the weaknesses of the Lab?

LS: Not as far as I am concerned but I am somewhat unusual in that I have never needed anything. I have been very lucky. I am 66 now.

For some or other I applied for a renewal of my grant and I got it, so I don't think that I am ever going to retire, but I have never needed anything. And I was very lucky.

SM: Now, when I spoke with George Snell, he said that he probably would not have retired.

LS: Really!

SM: Well, he found he had to though, because - not that the Lab wanted him to retire- but he found that getting grant support became increasingly difficult the older he got.

LS: Really.

END OF SIDE ONE

SM: And it seems so unfortunate as I listened to him talk. But it doesn't necessarily have to do with the Lab.

LS: That is amazing since he rather recently got the Nobel Prize, not being able to get grant support. I think one weakness of the Lab probably because of my age but, for example, George Snell had no place to go here at the Lab. Dr. Little didn't have a place to come here when he retired and none of the emeritus staff had any space in the Lab at all until the Lab gave Tibby Russell and Chai and Charity Waymouth office space. Have you seen their office? It is awful! Awful! It's a closet and I think that is really sad. I think it is sad that they had to have a place like that.

SM: It certainly isn't like this elsewhere. I mean if you are at a big university you can retire but still have at least a

small lab.

LS: That's what I thought, but I talked to Barbara Sanford about that and she said she doesn't know any place that provides any space for emeritus staff.

SM: Well, I just did a big oral history involving Francis Schmitt at MIT and boy, he's got a palace down there.

LS: Really. Ya, I don't think that is true that other places don't provide office space to emeritus staff.

SM: I don't know if they do it as a standard. Maybe you do have to provide some...for yourself and all that stuff, but Schmitt certainly has a nice plush place.

LS: Ya, most people are not at all happy with that place.

I was talking about it with Tibby and she said, maybe we feel we deserve more as we almost created this place.

SM: Well, when you are used to doing something for so many years and then suddenly have to stop, I mean I think what I see most people doing now, most scientists doing now, is phasing into the kind of research where they can just read and write, as opposed to actually having to have the bench.

LS: Right. And it should be a nice place, it would be nice to have a place to do that here at the Lab because you have to use the library. And I have never liked working at home.

SM: Most scientist don't, I think, because they are not used to it.

LS

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LS: I have retired 20%. I don't work Mondays which I like very much. But I am always ready to come back on Tuesday. And I will continue to do that for a while because I certainly do enjoy what I do here at the Lab. I also enjoy three-day weekends. Great.

SM: Oh, it's nice.

LS: You can really get projects done.

SM: That is right. And yet you are still four days here, so you are not really out of the ... So if you had a magic wand and you could wave it however you please, one thing you would do at Lab--you would arrange an emeritus wing.

LS: I think so.

SM: What else would you do? If you could have anything you wish, just wave a wand and make the Lab however you please.

LS: Well, I tell you as I have said I have always had whatever I want. But maybe it is because I don't want very much. I am satisfied. I have always had everything I wanted. always felt very lucky.

SM: What you need now, what you are looking for now is another breakthrough.

LS: Yup, I need an idea. They are the hardest things to come by.

SM: But they always seem to come, don't they?

LS: But I am still working on the last breakthrough yet. That is

still keeping me very busy. But in a while I'd like a new one.

SM: Well, I suspect what you probably have to do is just keep your eyes active, keep your ears open and-

LS: Yup, and about every five years, something happens. It is great and I am due.

SM: And you are due. Well, good--that is all you have to say. Anything else that you can think of in terms of funny stories or things that have happened or ...

LS: I can think of a lot of funny stories but - I have always felt extremely lucky to be able to work here. I have always enjoyed it.

SM: Who was the most colorful character you have worked with here?

LS: Maybe you had better stop the machine for a minute.

...Bill Murray was very much a character. I always enjoyed being with him. Henry Winn was a very colorful character. He was always a lot of fun to be with. We have a small group known as the Jackass Club and it is composed of the Chais and the Colemans, and Kandutschs and the Winns. And Stevenses. We have a dinner about once every few months. Get together for dinner and they were good times. We once invited somebody else, another couple and the word that got back to us that the conversation was jackassy. So we have since called ourselves the Jackass Club.

LS

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SM: I see.

LS: But that's really nice. We have known these people for so long.

SM: Now, Henry Winn left and went to Mass General.

LS: Mass General or Harvard.

SM: Yes, that's right. That's right because I remember calling him yesterday to set up an interview. Right.

Well, that's nice. Can you think of anything else?

LS: Nope.

SM: Well, thank you very much.

END OF INTERVIEW